

# Youth training programs beyond employment. Evidence from a randomized controlled trial. \*

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## Abstract

Youth unemployment is a pervasive phenomenon in Latin America and the Caribbean. Governments have widely used training programs in order to mitigate such problem. This paper documents the effects of a training program for low income youths, which comprises vocational training, life skills and work experience. Results show large gains in employment, with effects that remain more than two years after the intervention. The program shows also substantial effects on access to credit. Program participants exhibit a higher probability of having requested formal consumer credit, and a higher probability of having bank debts in good standing. The evidence suggests that our results are driven mainly by men and younger participants, who have higher gains in terms of outcomes, contrary to previous evidence from Latin America.

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# 1 Introduction

Youth unemployment is a pervasive phenomenon in Latin American and the Caribbean (henceforth LAC). Youth unemployment rates triple those of adults and labor informality is the rule (Gasparini et al., 2011). Young people have little work experience and insertion in the labor market is difficult (e.g. Pallais, 2013). In the past, governments and aid agencies have explored training programs as a potential solution to this problem, following the long tradition of active labor market policies carried out in OECD countries. There is a long history of training programs' evaluations, including randomized controlled trials like *National Supported Work* (NSW) and *Job Training Partnership Act* (JTPA).

Card's (Card et al., 2010) meta-study finds a modest impact at best for programs in Europe and the US. Generally, better results are found for women, and for older workers. Also, firm-based training is often more effective than classroom training. Moreover, programs that have links to work experience in the private sector tend to be more effective than public sector-based programs. The literature of evaluation of training programs in Latin America shows larger effects than in other regions. Unfortunately, such effects are sometime driven by weak methodological approaches and the quality of the data used.

As pointed out in Attanasio et al. (2011), average level of skills in developing countries is lower than in developed countries, so returns to skills may be higher. Also, specialized skills tend to be very valuable, since access to good jobs in the formal sector is generally limited to educated workers. González-Velosa et al. (2012) review the evidence available for LAC, highlighting the fact that many programs have quasi-experimental evaluations. There are few experimental evaluations available, and they show little or no effects.

In the case of Colombia the program *Jóvenes en Acción*, implemented in 2005, was evaluated in Attanasio et al. (2011), this program was aimed to unemployed young people between 18 and 25, from poor households. The training was offered randomly to men and women and it consisted of six months of vocational training. The evidence suggests that the program had positive effects in women' wages (an increase of 19.8%), and is more likely to have a job in the formal sector or one with written contract, for men the effects of the program on the quality of employment are small.

In the Dominican Republic exists experimental evaluations of the program *Juventud y Empleo* (cohort 2004). This program was aimed to young people with less than secondary education, between 18 and 29 years. It's main goal was to increase the employability of vulnerable young through basic training and technical/vocational courses. The evaluation of the program was conducted in

Card et al. (2011),<sup>1</sup> the results indicate no significant impact on employment, a modest impact on wages, conditional on having a job, which is a result coincident with those obtained in developed countries.

This paper attempts to provide more experimental evidence for job training programs in LAC for disadvantaged young, evaluating *entra21* a program carried in Córdoba, Argentina, but administered in several LAC countries. *entra21* differs from the above mentioned training programs in the sense that it is smaller, usually run by NGOs (usually business associations) and, while governments can participate in the implementation, it is not a country-wide program. Also, the cost of the program tends to be higher than the cost of government programs. Finally, the private sector has a higher involvement with the program.

Most existing literature looks at employment and wages as expected outcomes of training programs. However, we explore an additional link that goes from employment to credit access. Credit access is also a relevant outcome, since it helps to reduce risk, which is a central feature in the life of vulnerable groups. If *entra21* had a positive impact on employment and employment increases access to credit, then the effects of training go beyond the ones currently explored by the literature. The expected outcomes of this program are in line with all training programs, i.e. an increase in the probability of employment, but also we are interested in exploit this experiment beyond its stated purposes, we estimate the impact of the program on access to credit and welfare dependency.

The contributions from this paper are threefold. Firstly, it provides more evidence on the impact of youth training program in developing countries, which adds to the literature of active labour market policies (ALMP) in general. Secondly, we have a small sample size, but we use administrative records that imply large periods and precision in measurement. This is not commonly used in developing countries. Finally, we exploit experiment random assignment beyond its stated purposes exploring other related outcomes such as welfare dependency for women and access to credit. For the specific case of acquiring training, credit constraints would reduce demand for training, and thus motivate interventions. Most studies looking at access to credit have focused on the credit employment channel, via entrepreneurship and micro-finance. Here, the channel seems to be through more stable employment, specially for the younger group. However, we cannot rule out other channels through which the program may have an impact on credit.

This paper is organized as follows: Section 2 describes the program, Section 3 describes the random assignment process and the data used. Section 4 presents the results, Section 5 shows a cost-benefit analysis of the program and finally the Section 6 concludes.

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<sup>1</sup>Also see Ibararán et al. (2012).

## 2 Program Description

*entra21* is a program that targets specifically vulnerable, unemployed youths who have finished high school. *entra21* differs from the traditional training programs implemented in LAC countries in the sense that it is smaller, usually run by NGOs (mainly business associations) and, while governments can participate in the implementation, it is not a country-wide program. Furthermore, the number of training hours is greater than those of most training programs in LAC. Finally, one of the hallmarks of this type of program is the private sector's very active involvement in its various project components. Also, program administrators highlight that *entra21* increases the probability of finding "good quality" jobs in the formal sector, helping to reduce informality, which is very high among young and vulnerable individuals. The main disadvantage is the relative high cost *vis à vis* other training programs.

One of the *entra21* projects was conducted in Córdoba (Argentina) and the agencies in charge of the implementation were ADEC<sup>2</sup> and the *Secretaría de Desarrollo Social* (SDS). ADEC and the municipal and provincial governments of Córdoba worked alongside civil society organizations to implement Phase II of *entra21* program in the province. The program was financed by the Multilateral Investment Fund in the US. An experimental impact assessment strategy was designed to measure the ways in which the training influenced a number of different socioeconomic variables.<sup>3</sup>

The criteria for becoming eligible was set by the SDS. In order to become eligible, individuals must be unemployed within the ages 18-30, have a high school diploma and a total family income below the poverty line. The population eligible for the cohort comprised 407 young people. They were selected after a personal visit of the municipality/secretariat, which administered a survey (baseline). SDS gathered household socio-economic information for each participant. A total of 220 would be randomly assigned to treatment in a public lottery. Only the first cohort that participated from the program was assigned randomly and the individuals assigned to the control group could not participate of further calls.

Program administrators highlighted the fact that *entra21* was different from most traditional ALMP for the youth. First of all, the private sector was involved in the implementation. Secondly, the training had several components. It started with classroom training in an "*oficio*" (skill) which resulted from actual firms' demand. It also had some hours of training ICTs and life skills. Finally, the participants would do an internship to acquire on the job skills. Courses in different "oficios"

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<sup>2</sup>Agencia para el Desarrollo de Córdoba, [www.adec.org](http://www.adec.org)

<sup>3</sup>The program finished in 2012.

were offered.<sup>4</sup> Courses lasted for 884 hours divided in: 100 hours of technical in-class training, 64 hours of life skills training, 704 hours of internship and 16 extra hours which varied from basic skills to extra in-class technical training according to each type of course. Classroom training took place between mid-November and February 2011, and then were followed by an internship.

### 3 Data Description

This section explains how the random assignment was done, then describes the baseline information that was used in order to characterize individuals pre treatment. To estimate the impact of the program we use administrative records, because of in the Southern hemisphere December (when the follow up questionnaire was gathered) coincides with the start of the summer holidays and the capacity of SDS was limited and they collected little information.

#### 3.1 Random Assignment

*entra21* had an evaluation component that was conceived since the inception of the intervention. During its initial phase, the original eligible cohorts would be randomized into treatment and control groups in order to conduct a proper impact evaluation. All the participants entered a lottery and only the lottery winners would participate from the program. The 220 lottery winners and the 187 losers would be informed of the results<sup>5</sup> and training would start soon after that.

The lottery took place on November 9th, 2010. All the process was conducted in a very “transparent” manner, with public officials and a notary present. Eligible individuals who participated from the lottery were aware of the assignment into treatment mechanism and all agreed. We define as pre treatment period before third quarter 2010 (Q3-2010) and post treatment since Q1-2011.

Out of a total of 220 assigned to treatment, only 178 participated in the program. The 42 remaining either declined participation at the time of the beginning of the training or was not reachable by phone.

#### 3.2 Baseline

Baseline, pre treatment information can be obtained from the program’s application form, which was administered by the SDS. While the baseline was being collected, participants gave consent to

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<sup>4</sup>Courses were offered in the following fields: cookery, sales and administration and factory workers (“*operarios*”).

<sup>5</sup>Potential beneficiaries could all be present in the lottery. However, none of them was present at the time of the lottery.

tracking. Table 1<sup>6</sup> shows descriptive statistics and balance between treatment and control groups. In Panel A we observe all individual characteristics are balanced between treatment and control groups, which is an expected result in a successful randomization. As it can be observed, around one third of the program participants are male, average age is 23, most of them are single (approximately 70%) and 70% have complete high school education or less.

### 3.3 Administrative Data

In order to measure the impact of the program on the expected outcomes, we resort to three different sources of information. For employment information we have administrative data. Such data consists of monthly registered employment (from January 2003 to June 2013) and gross labor earnings (from January 2003 to June 2012) from SIPA (Sistema Integrado Previsional Argentino). Welfare participation, measured by being a beneficiary of *Asignación Universal por Hijo* (AUH), the Argentine Conditional Cash Transfer is available from ANSES (Administración Nacional de Seguridad Social). Credit in good/bad standing status is registered in the Central Bank of Argentina (Central de Deudores del Banco Central). Credit inquiries were obtained from credit rating agencies (Equifax-Veraz and NOSIS).

In Table 1 (Panel B) when pre-treatment outcomes are analyzed, we looked at employment at the 11 quarters before the program started. For 8 out of 11 quarters the means of the treatment and control group are not statistically different. For the last 2 quarters of 2009 and the first quarter of 2010 employment is higher in the treatment group. In terms of receiving welfare programs, there is between 10 and 15% of recipients of *AUH* in both groups, there is no difference between the two groups either. To observe credit use we define two measures: if a individuals have credit in good standing (regular credit) and the number of credit inquiries that companies (financials or not) request to a rating agency to know the credit situation when a individual wants a credit. For this variables there are not differences statistically significant pre treatment. Finally, we take into account some aggregated variables of real wages (with January 2011 as the base month). As can be seen there are some evidence of significant differences, but barely at 10%.

In terms of power calculations, such small sample would only allow us to detect effects above 8 percentage points in employment, which amounts to an effect size of 0.30.

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<sup>6</sup>Appendix A explains how the variables were constructed. Appendix B shows tables.

## 4 Estimated Effects

We conducted several estimations to look at a comprehensive set of outcomes. First, we estimated OLS regressions to obtain the estimator *Intention to Treat* (ITT) with and without covariates. Also, we calculated the estimator *Treatment on the Treated* (TOT) using the offer of training as instrument for training<sup>7</sup> (Angrist et al., 1996; Kling et al., 2007).

### 4.1 Employment and Wages

The main limitation of the administrative data available for employment is that it only registers formal employment. In this sense, results are not directly comparable to other evaluations in LAC. However, the main objective of the program was increase formal employment. Also, formal employment will be the most likely source of credit market effects.

In Graph 1<sup>8</sup> we consider only the evolution of employment rates by random assignment, from Q1-2008 to Q2-2013, it can be observed there was an increase in employment rates for both groups at the end of 2010. To calculate the effect of the program for each quarter the ITT estimator was estimated without covariates, the results are shown in Graph 2. In Graph 3 we also include as a control an average of pre treatment quarters from Q1-2008 to Q3-2010. In both graphs there are three quarters statistically significant (as can be seen in Table 1) although the allocation to each group was done randomly.

Table 2 presents the impact of the program on employment but in aggregated outcomes post treatment. We conducted two estimations, intention to treat (ITT) and treatment on the treated (TOT), instrumenting completed training with random assignment. We look at three outcomes: *Average of employment* for Q1-2011 to Q2-2013, *Always employed* and *Employed in multiple periods* (time series), with and without covariates. In this case standard errors were clustered at the individual level (Angrist and Pischke, 2008). ITT and TOT estimates can be observed in Panels A and B respectively. For the ITT, we find positive and statistically significant program effects for the three outcomes considered. When we add covariates *Always employed* is not significant. When we look at the TOT, effects are much higher, almost twice as much.

To describe how the results are changing when we add a new quarter in our outcomes, in Graph 4 and 5 we add progressively a new quarter to generate the final outcome in each considerer quarter. In the case of average of quarters (Graph 4) the impact of the program is statistically significant

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<sup>7</sup>Random assignment as IV for training completion; n=106 out of 220, p=0.481.

<sup>8</sup>Appendix C.

over the period under analysis, but it has slightly lower magnitudes when we increase the number of quarters, indicating that the effect may be dissipating over time.

Also we are interested in estimating the impact of the program in different groups (Djebbari and Smith, 2008). We repeated the estimations for men, women and for 18-24 and 25-30 years. Table 3 shows ITT estimates of impact on employment by gender and age groups. As it can be appraised, effects are non-statistically significant for women and older individuals (25-30 years of age), while positive and significant for men and younger individuals, aged 18-24. Results also holds when calculating estimates of TOT (Table 4).

It is also interesting to analyze the impacts of the program with respect to stay in the same work. We build variables that indicate whether the individual works for the same employer from one quarter to another. Table 5 shows results on attachment, we found an increase in post treatment attachment that is statistically significant only for male.

Another important result are wages, to analyze the impact of the program on real wages (with January 2011 as the base month) Graph 6 shows the evolution of wage from Q1-2008 to Q2-2012 by random assignment, while Graph 7 shows the differences between groups, showing differences statistically significant for posttreatment quarters.

Panel A in Table 6 reports ITT estimates for the results on earnings differences between the treatment and control groups, including the zeros. We observed significant increases in post treatment wages (considering *Sum* and *Average* of wage). Increase in earnings can be caused by increase in employment or by increase in earnings of those employed. Total impact is a combination of productivity gains and changes in employment composition. We calculate bounds for earnings because wages zero are included, this was done in other studies such as Lee (2008), Blanco et al. (2011) and Flores and Chen (2012). Here we follow Attanasio et al. (2011) to estimate the program's impact on productivity.<sup>9</sup> They divide the sample of individuals in four groups: those who work regardless of the program (or always takers in terms of Angrist and Imbens, 1994), those who would never work, those who are switching to work because of the program (the compliers) and those switching out of work because of the program. Randomization ensures that the size of each group is independent of the assignment to treatment. In first place the monotonicity assumption is used, indicating that individuals who would work without the program would work if they did the training, this allows to decompose the effect of the program on the earnings of compliers plus the always takers, and estimate the productivity gain of the program and the change in composition. Table 5, Panel A shows that *Average* earnings did increase, but we cannot conclude whether it is

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<sup>9</sup>The Appendix B of Attanasio et al. (2011) presents a derivation of such bounds.



due to productivity gains or change in employment. The bounds are estimated by estimating the productivity effects and the distribution of wages in the control group (Panel C). The calculated bounds for the average wages are big and the lower bound is negative. But an additional assumption is considered to narrow this lower limit, nonprogram earnings of those who always work are at least as high as the nonprogram earnings of individuals who are no longer unemployed. The bounds with this assumption are limited now between AR\$108 and AR\$919.

## 4.2 Credit Use

In this literature, credit constraints would reduce demand for training and thus motivate interventions. Studies looking at credit use have focused on the credit-employment channel (entrepreneurship, etc.). The relevance of the credit use as an outcome in this study is related to evidence for LAC that finds effect of training programs on formal employment. If we see a simple model of credit, higher-less volatile-more verifiable earnings in  $t=0$  would increase borrowing in  $t=1$  even in the absence of credit constraints. In this paper we are also interested in the effects of the program on credit use, for this reason we use two measures that indicates credit use or access to credit:

- credit in good standing: equals 1 if individual has banking credit in good standing, 0 otherwise (bad or no credit).
- credit inquiries: number of credit inquiries to credit rating agency. These inquiries appears when an individual requests some form of banking or consumer credit and the institution requests information about her.

In this section, the interest is to estimate the direct effect of the training offer (ITT), then the effect of employment on credit and finally the effect of earnings on credit. In first place Table 7 and 8 show the results for credit in good standing pre and post treatment, respectively. The outcomes of interest are the *Sum* of quarters, *Average*, *Always regular* and *Regular multiple periods* (the quarters included depend on the period). Panel A shows the direct impact of training offer on these measures of interest, there are no pre treatment significant results but in Table 8, except *Always regular*, the rest of the variables are positive and statistically significant. When we consider the direct impact of employment (using *Average employment* pre or post treatment according each period) or wages (using the *Sum of wages* over 1000, pre or post treatment) on credit using OLS the variables are

statistically significant post treatment but also they were significant prior to the implementation of the program, so we cannot conclude about the impact of the program in these cases. Only when we use the random assignment as an instrument for *Average employment*, *Multiple periods* (for credit in good standing) is post treatment statistically significance.

For ITT estimations the sample was divide in four groups: female, male, and 18-24 and 25-30 years to determinate which group could be conducting the results found previously. The results are in Table 9 and again we observed that only for men and younger participants there are positive and significative effects.

In Tables 10 and 11 the dependent variable of interest is the number of credit inquiries before and after treatment, respectively, we analyze the *Sum*, their *Average* and *Multiple periods*. When we analyze the impact of random assignment note that pre treatment variables were not statistically significant, but the result is positive and statistically significant post treatment. Analyzing the direct impact of employment or wages again there are statistically significant differences pre and post treatment. When the employment (average) is instrumented with the random assignment we found positive and statistically significant effects for all variables considered of the number of credit inquiries (only post-treatment, Table 11).

For this variables we also consider heterogeneous impact by estimating ITT (Table 12) and as can be seen the evidence suggests that there are positive and significant effects only for male and younger participants.

### 4.3 Welfare Dependancy

Another output of interest is the effect of training on welfare dependency in particular for women, measured by receiving AUH benefits. We construct a dummy that takes value 1 if a women is beneficiary of the AUH. The results are shown in Table 13. While 12% of control group received benefits during all the periods in the sample, the proportion increases to 22% when considering multiple periods. Welfare recipients tend to be concentrated among older participants, and we only find a significant negative effect for younger women, indicating that the training has a deterrent effect on participation in such benefits.

## 5 Cost Benefit analysis

One difference between *entra21* and other training program in LAC is its cost. *entra21* is significantly more expensive than programs targeting the same beneficiary groups. The implementing agency states that the quality of the training provided in *entra21* is superior to that of those traditional government run programs and this justifies its higher costs. Roughly the cost of operation per trainee was US\$ 1722 (more than double the Colombian and the Dominican Republic program with costs of US\$750 and US\$330 respectively). The last column of Table 5 shows an average quarterly gain (due to employment and earnings gains) of AR\$ 280, or AR\$ 1123 per year. Using a 5% discount rate with no depreciation, we obtain an internal rate of return of 19%, lower than similar programs in the region.

## 6 Conclusions

Youth unemployment is a pervasive phenomenon in Latin American and the Caribbean and training programs are considered by governments and aid agencies as a potential solution but in the region there is limited causal evidence to conclude about their effectiveness.

This paper is a contribution to the evaluation of training policies in LAC, documenting the effects of a training program for low income youths named *entra21* in the province of Córdoba in Argentina. *entra21* provided a combination of vocational training, life skills training and work experience. Assignment to the program was random, and thus allowing us to obtain causal estimates of training offer on the outcomes of interest. One of the main challenges of the evaluation is the small sample size, resulting from the fact that the program was small too. We made use of administrative data, available before and after the treatment.

We find positive and statistically significant effects on employment, which persist up to two years after the program. Besides these labor market impacts, the evidence also indicates a small reduction in welfare participation for younger women. When we look at gains in earnings, there are positive and significant effects too caused both by productivity gains and increase in employment. Bounds were estimated in order to give some idea of the productivity gain. However, such bounds do include the zero gains, so we cannot conclude that the gain was caused by a productivity increase. Also, there are effects on credit, credit in good standing and credit inquires. For the specific case of acquiring training, credit constraints would reduce demand for training, and thus motivate interventions. Most studies looking at access to credit have focused on the credit employment

channel, via entrepreneurship and micro-finance. Here, we conjecture that the channel seems to be through more stable employment, specially for the younger group.

Besides the mentioned before is interesting to note that gains in outcomes analyzed when we split the sample are significant for men and for younger individuals (18-24 versus 25-30), which is a result contrary to previous evidence from Latin America.

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# Appendix A

## Definition of Variables

**Random assignment:** indicates the random assignment results to participate in the training (takes value 1 for the treatment group, 0 for the control group).

**Completed training:** dummy that indicates if the training is complete.

**Formal employment** (Table 2, 3 and 4): dummy by quarter that takes value 1 if a person was employed at least one month in the quarter.

- *Average of Q1-2011 to Q1-2013:* average of quarters of the formal employment variable.

- *Always employed Q1-2011 to Q1-2013:* dummy that takes value 1 if the person was employed during all the considered period, 0 in other case.

- *Multiple periods Q1-2011 to Q1-2013:* time series (by quarter) of formal employment variable.

**Attachment** (Table 5): dummy that takes value 1 if a person has the same employee in the considered periods.

- *Q3-2010 to Q4-2010:* dummy that takes value 1 if a person has the same employee in Q3-2010 and Q4-2010.

- *Q1-2011 to Q4-2011:* dummy that takes value 1 if a person has the same employee in Q1-2011 y en el Q4-2011.

- *Q1-2011 to Q2-2012:* dummy that takes value 1 if a person has the same employee in Q1-2011 and Q2-2012.

**Regular credit** (Table 7, 8 and 9): dummy by quarter that takes value 1 in case that the individual credit status is in good standing (value 1 or 2, according to the classification of Central Bank of Argentina BCRA), 0 in other case.

- *Sum of Q2-2010 to Q3-2010 & Sum of Q1-2011 to Q3-2012:* sum of Regular Credit variable in each of the considered periods.

- *Average of Q2-2010 to Q3-2010 & Average of Q1-2011 to Q3-2012:* average of quarters of the Regular Credit variable in each of the considered periods.

- *Always regular Q2-2010 to Q3-2010 & Always regular Q1-2011 to Q3-2012:* dummy that takes value 1 if the individual has a credit in good standing during all the considered period.

- *Multiple periods Q2-2010 to Q3-2010 & Multiple periods Q1-2011 to Q3-2012:* time series (by quarter) of Regular Credit variable in each of the considered periods.

**Number of Credit Inquiries** (Tables 10, 11 and 12): variable that indicates the number of VERAZ reports that were required (by financial sector, non-financial and others) to find out the individual credit situation.

- *Sum of Q1-2008 to Q3-2010 & Inquiries Sum of Q1-2011 to Q3-2012*: sum of Number of Credit Inquiries variable in each of the considered periods.

- *Average of Q1-2008 to Q3-2010 & Inquiries Average of Q1-2011 to Q3-2012*: average of quarters of Number of Credit Inquiries variable in each of the considered periods.

- *Multiple periods Q1-2008 to Q3-2010 & Multiple periods Q1-2011 to Q3-2012*: time series (by quarter) of Number of Credit Inquiries variable in each of the considered periods.

**Asignación Universal por Hijo** (AUH - Table 13): dummy by quarter that takes value 1 if a woman perceived social assistance.

- *Always Q1-2011 to Q1-2013*: dummy that takes value 1 if a woman perceives the AUH during all the considered period.

- *Multiple periods Q1-2011 to Q1-2013*: time series (by quarter) of Asignación Universal por Hijo variable.



# Appendix B

## Tables

Table 1.  
Pre treatment summary statistics.

	Treatment			Control			Difference		
	Mean (1)	Standard Error	N	Mean (2)	Standard Error	N	(1)-(2)	Standard Error	P-value
<i>Panel A. Individual characteristics</i>									
Male	0.2955	0.0308	220	0.3369	0.0347	187	-0.0414	0.0463	0.3707
Age	23.5455	0.2377	220	23.7968	0.2631	187	-0.2513	0.3540	0.4781
Incomplete elementary school	0.0364	0.0126	220	0.0267	0.0118	187	0.0096	0.0175	0.5832
Complete elementary school	0.0773	0.0180	220	0.0535	0.0165	187	0.0238	0.0248	0.3376
Incomplete high school	0.2773	0.0302	220	0.3262	0.0344	187	-0.0489	0.0456	0.2841
Complete high school	0.3273	0.0317	220	0.3316	0.0345	187	-0.0043	0.0469	0.9273
Incomplete tertiary level/college	0.1773	0.0258	220	0.1604	0.0269	187	0.0168	0.0374	0.6527
Complete tertiary level/college	0.0682	0.0170	220	0.0588	0.0173	187	0.0094	0.0244	0.7013
Missing education	0.0364	0.0126	220	0.0428	0.0148	187	-0.0064	0.0194	0.7407
Children in the household	0.1909	0.0266	220	0.2460	0.0316	187	-0.0551	0.0410	0.1794
Single/Widower	0.6955	0.0311	220	0.6684	0.0345	187	0.0270	0.0464	0.5607
Married/Cohabiting	0.2318	0.0285	220	0.2727	0.0327	187	-0.0409	0.0432	0.3438
Divorced/Separated	0.0455	0.0141	220	0.0374	0.0139	187	0.0080	0.0199	0.6878
Missing marital status	0.0273	0.0110	220	0.0214	0.0106	187	0.0059	0.0154	0.7033
<i>Panel B. Pre treatment outcomes</i>									
Employment quarter 1 2008	0.1409	0.0235	220	0.1027	0.0224	185	0.0382	0.0328	0.2454
Employment quarter 2 2008	0.1682	0.0253	220	0.1189	0.0239	185	0.0493	0.0352	0.1623
Employment quarter 3 2008	0.1500	0.0241	220	0.1297	0.0248	185	0.0203	0.0348	0.5601
Employment quarter 4 2008	0.1545	0.0244	220	0.1297	0.0248	185	0.0248	0.0350	0.4788
Employment quarter 1 2009	0.1545	0.0244	220	0.1081	0.0229	185	0.0464	0.0339	0.1717
Employment quarter 2 2009	0.1545	0.0244	220	0.1243	0.0243	185	0.0302	0.0347	0.3849
Employment quarter 3 2009	0.1773	0.0258	220	0.0973	0.0218	185	0.0800	0.0345	0.0211**
Employment quarter 4 2009	0.2045	0.0273	220	0.1027	0.0224	185	0.1018	0.0361	0.0050***
Employment quarter 1 2010	0.2000	0.0270	220	0.1135	0.0234	185	0.0865	0.0365	0.0181**
Employment quarter 2 2010	0.1727	0.0255	220	0.1243	0.0243	185	0.0484	0.0357	0.1758
Employment quarter 3 2010	0.1364	0.0232	220	0.1081	0.0229	185	0.0283	0.0329	0.3905
Employment quarter 4 2010	0.1955	0.0268	220	0.1730	0.0279	185	0.0225	0.0388	0.5629
Average of quarters empl. (Q1 2008-Q3 2010)	0.1649	0.0188	220	0.1145	0.0170	185	0.0504	0.0258	0.0512*
Always employed (Q1 2008-Q3 2010)	0.0318	0.0119	220	0.0108	0.0076	185	0.0210	0.0147	0.1539
AUH quarter 4 2009	0.1500	0.0241	220	0.1514	0.0264	185	-0.0014	0.0358	0.9699
AUH quarter 1 2010	0.1364	0.0232	220	0.1514	0.0264	185	-0.0150	0.0350	0.6689
AUH quarter 2 2010	0.1364	0.0232	220	0.1351	0.0252	185	0.0012	0.0343	0.9714
AUH quarter 3 2010	0.1318	0.0229	220	0.1081	0.0229	185	0.0237	0.0326	0.4673
AUH quarter 4 2010	0.1318	0.0229	220	0.1351	0.0252	185	-0.0033	0.0340	0.9223
Average of quarters AUH (Q1 2010-Q3 2010)	0.4045	0.0646	220	0.3946	0.0695	185	0.0100	0.0950	0.9166
Always AUH (Q1 2010-Q3 2010)	0.4045	0.0646	220	0.3946	0.0695	185	0.0100	0.0950	0.9166
Regular credit quarter 2 2010	0.0955	0.0199	220	0.0642	0.0180	187	0.0313	0.0272	0.2503
Regular credit quarter 3 2010	0.0818	0.0185	220	0.0588	0.0173	187	0.0230	0.0256	0.3701
Regular credit quarter 4 2010	0.0864	0.0190	220	0.0588	0.0173	187	0.0275	0.0260	0.2905
Average of quarters reg. cred. (Q2 2010-Q3 2010)	0.0894	0.0187	218	0.0632	0.0174	182	0.0263	0.0259	0.3116
Always regular credit (Q1 2010-Q4 2010)	0.0780	0.0182	218	0.0549	0.0169	182	0.0230	0.0252	0.3617
Average credit inquiries (Q1 2008-Q3 2010)	0.1260	0.0124	220	0.1320	0.0128	186	-0.0059	0.0179	0.7405
Sum real earnings 01-2003 to 12-2010/1000	16.9049	2.6025	220	10.6690	2.2053	187	6.2359	3.4789	0.0738*
Sum real earnings 01-2008 to 12-2010/1000	10.2328	1.4816	220	6.6160	1.2699	187	3.6167	1.9883	0.0697*
Sum real earnings 01-2008 to 09-2010/1000	9.3897	1.4207	220	6.0390	1.2283	187	3.3507	1.9124	0.0805*

Notes: AUH means Asignación Universal por Hijo.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 2.  
Effects on formal employment. All sample.

	Without covariates			With covariates		
	Average of Q1-2011 to Q2-2013	Always employed Q1-2011 to Q2-2013	Multiple periods Q1-2011 to Q2-2013 (a)	Average of Q1-2011 to Q2-2013	Always employed Q1-2011 to Q2-2013	Multiple periods Q1-2011 to Q2-2013 (a)
<i>Panel A. ITT (OLS)</i>						
Random assignment	0.0835 [0.0365]**	0.0553 [0.0307]*	0.0835 [0.0365]**	0.0714 [0.0349]**	0.0452 [0.0302]	0.0890 [0.0346]**
Constant	0.2692 [0.0255]***	0.0811 [0.0201]***	0.2692 [0.0254]***	-0.0230 [0.1019]	-0.1602 [0.0651]**	-0.0065 [0.0963]
Observations	405	405	4050	405	405	4050
R-squared	0.01	0.01	0.01	0.24	0.15	0.1
Control group mean	0.2692	0.0811	0.2692	0.2692	0.0811	0.2692
<i>Panel B. TOT (IV-Completed training instrumented by random assignment)</i>						
Completed training	0.1734 [0.0759]**	0.1147 [0.0640]*	0.1734 [0.0758]**	0.1493 [0.0725]**	0.0945 [0.0635]	0.1839 [0.0715]**
Constant	0.2692 [0.0255]***	0.0811 [0.0201]***	0.2692 [0.0254]***	0.0087 [0.0968]	-0.1401 [0.0672]**	0.0379 [0.0876]
Observations	405	405	4050	405	405	4050
R-squared	0.01		0.01	0.24	0.15	0.09
Control group mean	0.2692	0.0811	0.2692	0.2692	0.0811	0.2692

Notes: Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. *Average* and *Always employed* also include pre treatment formal employment Q1-2008 to Q3-2010. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 3.

Effects on formal employment ITT (OLS). Heterogeneous impact.

	Average of Q1-2011 to Q2-2013	Always employed Q1- 2011 to Q2- 2013	Multiple periods Q1- 2011 to Q2- 2013 (a)	Average of Q1-2011 to Q2-2013	Always employed Q1- 2011 to Q2- 2013	Multiple periods Q1- 2011 to Q2- 2013 (a)
	<i>Panel A. Female</i>			<i>Panel B. Male</i>		
Random assignment	0.0058 [0.0398]	-0.0216 [0.0304]	0.0196 [0.0392]	0.2287 [0.0693]***	0.1907 [0.0768]**	0.2337 [0.0671]***
Constant	0.0258 [0.0881]	-0.0553 [0.0506]	0.0429 [0.0900]	0.5586 [0.3627]	-0.2408 [0.2163]	-0.0351 [0.3812]
Observations	279	279	2790	126	126	1260
R-squared	0.23	0.18	0.07	0.31	0.27	0.11
Control group mean	0.2242	0.0806	0.2242	0.3607	0.0820	0.3607
	<i>Panel C. Age 18-24</i>			<i>Panel D. Age 25-30</i>		
Random assignment	0.1134 [0.0487]**	0.1050 [0.0451]**	0.1060 [0.0483]**	0.0111 [0.0527]	-0.0212 [0.0412]	0.0438 [0.0487]
Constant	0.1272 [0.0990]	-0.0741 [0.0671]	0.1008 [0.0839]	0.0801 [0.1033]	-0.0199 [0.0508]	0.0573 [0.0918]
Observations	229	229	2290	176	176	1760
R-squared	0.19	0.13	0.06	0.27	0.24	0.13
Control group mean	0.3127	0.0784	0.3127	0.2157	0.0843	0.2157

Notes: Covariates included are children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Average and Always employed also include pre treatment formal employment Q1-2008 to Q3-2010. Panel A and B also include year of birth (dummies. Panel C and D include male. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 4.

Effects on formal employment TOT (IV-Completed training instrumented by random assignment).  
Heterogeneous impact.

	Average of Q1-2011 to Q2-2013	Always employed Q1- 2011 to Q2- 2013	Multiple periods Q1- 2011 to Q2- 2013 (a)	Average of Q1-2011 to Q2-2013	Always employed Q1- 2011 to Q2- 2013	Multiple periods Q1- 2011 to Q2- 2013 (a)
	<i>Panel A. Female</i>			<i>Panel B. Male</i>		
Completed training	0.0125 [0.0856]	-0.0464 [0.0655]	0.0428 [0.0856]	0.4465 [0.1442]***	0.3723 [0.1589]**	0.4295 [0.1261]***
Constant	0.0295 [0.0875]	-0.0689 [0.0492]	0.0557 [0.0876]	-0.9261 [0.2196]***	-0.6237 [0.2593]**	0.7269 [0.2571]***
Observations	279	279	2790	126	126	1260
R-squared	0.23	0.18	0.07	0.27	0.23	0.10
Control group mean	0.2242	0.0806	0.2242	0.3607	0.0820	0.3607
	<i>Panel C. Age 18-24</i>			<i>Panel D. Age 25-30</i>		
Completed training	0.2280 [0.0993]**	0.2112 [0.0943]**	0.2206 [0.1022]**	0.0227 [0.1071]	-0.0431 [0.0848]	0.0858 [0.0946]
Constant	0.2256 [0.1144]**	0.0170 [0.0565]	0.1903 [0.0921]**	0.0835 [0.0962]	-0.0263 [0.0454]	0.0704 [0.0844]
Observations	229	229	2290	176	176	1760
R-squared	0.16	0.06	0.04	0.28	0.22	0.14
Control group mean	0.3127	0.0784	0.3127	0.2157	0.0843	0.2157

Notes: Covariates included are children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Average and Always employed also include pre treatment formal employment Q1-2008 to Q3-2010. Panel A and B also include year of birth (dummies). Panel C and D include male. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 5.  
Attachment (pre and post treatment).

	Pre treatment	Post treatment	
	Q3-2010 to Q4-2010	Q1-2011 to Q4-2011	Q1-2011 to Q2-2012
<i>Panel A. All sample</i>			
Random assignment	-0.0028 [0.0218]	0.0404 [0.0308]	0.0201 [0.0286]
Constant	-0.0082 [0.0386]	-0.1042 [0.0624]*	-0.0889 [0.0523]*
Observations	407	405	405
R-squared	0.06	0.13	0.11
Control group mean	0.0535	0.0802	0.0695
<i>Panel B. Female</i>			
Random assignment	0.0055 [0.0200]	-0.0467 [0.0323]	-0.036 [0.0304]
Constant	0.0282 [0.0259]	0.0128 [0.0546]	-0.0285 [0.0494]
Observations	279	279	279
R-squared	0.06	0.15	0.13
Control group mean	0.1111	0.0968	0.0806
<i>Panel C. Male</i>			
Random assignment	-0.0295 [0.0593]	0.1611 [0.0674]**	0.1246 [0.0631]*
Constant	-0.1709 [0.2490]	-0.4487 [0.2045]**	-0.236 [0.2045]
Observations	128	126	126
R-squared	0.11	0.33	0.30
Control group mean	0.1111	0.0476	0.0476

Notes: Covariates included are year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Also we include pre treatment formal employment Q1-2008 to Q3-2010. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 6.  
Wage bounds (post treatment).

	Average Wage Q1 2011	Average Wage Q2 2011	Average Wage Q3 2011	Average Wage Q4 2011	Average Wage Q1 2012	Average Wage Q2 2012	Sum earnings 01-2011 to 06- 2012/1000	Average earnings 01- 2011 to 06- 2012
<i>Panel A. ITT (OLS) Average Wage</i>								
Random assignment	183.5857 [109.7492]*	226.4163 [108.9388]**	233.9425 [127.9963]*	317.8133 [138.8779]**	344.5029 [152.2345]**	402.6596 [145.1452]**	5.3362 [1.9625]**	280.6479 [130.4095]**
Control group mean	420.0248	410.9003	598.8799	579.0033	613.3621	585.3484	8.3225	868.8511
<i>Panel B. ITT (OLS) Employment</i>								
Random assignment	0.0972 [0.0437]**	0.0858 [0.0421]**	0.0270 [0.0437]	0.0604 [0.0446]	0.1136 [0.0454]**	0.1134 [0.0455]**	0.1083 [0.0479]**	0.1083 [0.0479]**
Control group mean	0.2216	0.2378	0.2919	0.2649	0.2378	0.2378	0.4216	0.4216
<i>Panel C. Wage Bounds</i>								
Lower Bound (Only monotonicity)	-1647.4748	-1081.1813	-53.7965	-415.9526	-1578.1178	-1345.8914	-6.8884	-702.3218
Lower Bound (Attanasio et al)	-1.9201	241.7123	559.5941	571.3389	146.8613	351.9972	6.0379	108.6262
Upper Bound	1643.6346	1564.6059	1172.9846	1558.6305	1871.8404	2049.8857	18.9641	919.5742

Notes: Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Also we include pre treatment formal employment Q1-2008 to Q3-2010. Robust standard errors in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 7.  
Credit in good standing (pre treatment).

	Sum of Q2- 2010 to Q3- 2010	Average of Q2-2010 to Q3-2010	Always regular Q2- 2010 to Q3- 2010	Multiple periods Q2- 2010 to Q3- 2010 (a)
<i>Panel A. ITT (OLS)</i>				
Random assignment	0.0586 [0.0507]	0.0293 [0.0254]	0.0259 [0.0248]	0.0295 [0.0246]
Constant	0.4631 [0.3430]	0.2316 [0.1715]	0.2447 [0.1696]	0.2316 [0.1683]
Observations	400	400	400	814
R-squared	0.12	0.12	0.12	0.11
Control group mean	0.1264	0.0632	0.0549	0.0615
<i>Panel B. Direct effect of employment on credit (OLS)</i>				
DDJJ Average Q1-2008 to Q3-2010	0.6144 [0.1650]***	0.3072 [0.0825]***	0.2736 [0.0830]***	0.3065 [0.0809]***
Constant	0.4555 [0.3577]	0.2278 [0.1789]	0.2412 [0.1762]	0.2286 [0.1754]
Observations	400	400	400	810
R-squared	0.19	0.19	0.18	0.18
Control group mean	0.1264	0.0632	0.0549	0.0615
<i>Panel C. Direct effect of earnings on credit (OLS)</i>				
Sum real earnings 01- 2008 to 09-2010/1000	0.0092 [0.0024]***	0.0046 [0.0012]***	0.0043 [0.0012]***	0.0046 [0.0012]***
Constant	0.4444 [0.3633]	0.2222 [0.1817]	0.2348 [0.1789]	0.2226 [0.1783]
Observations	400	400	400	814
R-squared	0.21	0.21	0.21	0.20
Control group mean	0.1264	0.0632	0.0549	0.0615
<i>Panel D. TOT (IV-DDJJ Average instrumented by random assignment)</i>				
DDJJ Average Q1-2008 to Q3-2010	0.9520 [0.7769]	0.4760 [0.3884]	0.4204 [0.3818]	0.4816 [0.3867]
Constant	0.4296 [0.3589]	0.2148 [0.1795]	0.2299 [0.1764]	0.2158 [0.1761]
Observations	400	400	400	810
R-squared	0.17	0.17	0.16	0.16
Control group mean	0.1264	0.0632	0.0549	0.0615

Notes: Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 8.  
Credit in good standing (post treatment).

	Sum of Q1- 2011 to Q3- 2012	Average of Q1-2011 to Q3-2012	Always regular Q1- 2011 to Q3- 2012	Multiple periods Q1- 2011 to Q3- 2012 (a)
<i>Panel A. ITT (OLS)</i>				
Random assignment	0.2461 [0.1433]*	0.0352 [0.0205]*	0.0179 [0.0140]	0.0474 [0.0230]**
Constant	1.7615 [0.8284]**	0.2516 [0.1183]**	0.1327 [0.1210]	0.3479 [0.1672]**
Observations	400	400	400	2442
R-squared	0.36	0.36	0.30	0.06
Control group mean	0.5989	0.0856	0.0165	0.0793
<i>Panel B. Direct effect of employment on credit (OLS)</i>				
DDJJ Average Q1-2011 to Q3-2012	0.9894 [0.2213]***	0.1413 [0.0316]***	0.0114 [0.0192]	0.1653 [0.0339]***
Constant	1.8944 [0.8517]**	0.2706 [0.1217]**	0.1436 [0.1222]	0.3681 [0.1711]**
Observations	400	400	400	2430
R-squared	0.40	0.40	0.30	0.09
Control group mean	0.5989	0.0856	0.0165	0.0793
<i>Panel C. Direct effect of earnings on credit (OLS)</i>				
Sum real earnings 01- 2011 to 06-2012/1000	0.0216 [0.0042]***	0.0031 [0.0006]***	0.0005 [0.0004]	0.0033 [0.0007]***
Constant	2.0003 [0.8329]**	0.2858 [0.1190]**	0.1459 [0.1220]	0.3826 [0.1676]**
Observations	400	400	400	2442
R-squared	0.42	0.42	0.30	0.10
Control group mean	0.5989	0.0856	0.0165	0.0793
<i>Panel D. IV (DDJJ Average instrumented by random assignment)</i>				
DDJJ Average Q1-2011 to Q3-2012	2.9969 [1.8911]	0.4281 [0.2702]	0.2181 [0.1945]	0.4562 [0.2399]*
Constant	1.8540 [0.9562]*	0.2649 [0.1366]*	0.1395 [0.1283]	0.3494 [0.1808]*
Observations	400	400	400	2430
R-squared	0.24	0.24	0.12	
Control group mean	0.5989	0.0856	0.0165	0.0793

Notes: Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Also we include pre treatment formal employment Q1-2008 to Q3-2010 and pre treatment regular credit Q2-2010 to Q3-2010. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.



Table 9.  
Credit in good standing ITT (OLS-post treatment). Heterogeneous impact.

	Sum of Q1- 2011 to Q3- 2012	Average of Q1-2011 to Q3-2012	Always regular Q1- 2011 to Q3- 2012	Multiple periods Q1- 2011 to Q3- 2012 (a)	Sum of Q1- 2011 to Q3- 2012	Average of Q1-2011 to Q3-2012	Always regular Q1- 2011 to Q3- 2012	Multiple periods Q1- 2011 to Q3- 2012 (a)
<i>Panel A. Female</i>				<i>Panel B. Male</i>				
Random assignment	-0.0084 [0.1544]	-0.0012 [0.0221]	0.0170 [0.0140]	0.0105 [0.0255]	0.8694 [0.3392]**	0.1242 [0.0485]**	0.0352 [0.0377]	0.1181 [0.0409]**
Constant	1.7963 [0.8280]**	0.2566 [0.1183]**	0.1419 [0.1215]	0.4041 [0.1753]**	-1.1644 [1.0193]	-0.1663 [0.1456]	-0.0836 [0.0964]	0.4022 [0.1574]**
Observations	277	277	277	1674	123	123	123	768
R-squared	0.42	0.42	0.45	0.05	0.47	0.47	0.35	0.19
Control group mean	0.6423	0.0918	0.0163	0.0847	0.5085	0.0726	0.0169	0.0688
<i>Panel C. Age 18-24</i>				<i>Panel D. Age 25-30</i>				
Random assignment	0.5243 [0.1826]***	0.0749 [0.0261]***	0.0068 [0.0123]	0.0682 [0.0269]**	0.0036 [0.2279]	0.0005 [0.0326]	0.0304 [0.0248]	0.0212 [0.0391]
Constant	-0.6770 [0.3532]*	-0.0967 [0.0505]*	-0.0166 [0.0255]	-0.0893 [0.0423]**	0.8478 [0.6397]	0.1211 [0.0914]	0.0394 [0.1140]	0.2942 [0.1606]*
Observations	224	224	224	1386	176	176	176	1056
R-squared	0.27	0.27	0.21	0.04	0.46	0.46	0.45	0.04
Control group mean	0.4242	0.0606	0.0101	0.0561	0.8072	0.1153	0.0241	0.1084

Notes: Covariates included are children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Panel A and B also include year of birth (dummies). Panel C and D include male. Also we include pre treatment formal employment Q1-2008 to Q3-2010 and pre treatment regular credit Q2-2010 to Q3-2010. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 10.  
Number of credit inquiries (pre treatment).

	Sum of Q1- 2008 to Q3- 2010	Average of Q1-2008 to Q3-2010	Multiple periods Q1- 2008 to Q3- 2010 (a)
<i>Panel A. ITT (OLS)</i>			
Random assignment	-0.0250 [0.1807]	-0.0023 [0.0164]	-0.0016 [0.0160]
Constant	2.3861 [0.6753]***	0.2169 [0.0614]***	0.2175 [0.0596]***
Observations	406	406	4477
R-squared	0.16	0.16	0.03
Control group mean	1.4516	0.132	0.1313
<i>Panel B. Direct effect of employment on inquiries (OLS)</i>			
DDJJ Average Q1-2008 to Q3-2010	3.2092 [0.4661]***	0.2917 [0.0424]***	0.2917 [0.0412]***
Constant	2.1354 [0.7104]***	0.1941 [0.0646]***	0.1941 [0.0628]***
Observations	405	405	4455
R-squared	0.30	0.30	0.05
Control group mean	1.4516	0.132	0.1313
<i>Panel C. Direct effect of earnings on inquiries (OLS)</i>			
Sum real earnings 01- 2008 to 09-2010/1000	0.0427 [0.0072]***	0.0039 [0.0007]***	0.0039 [0.0006]***
Constant	2.1043 [0.7498]***	0.1913 [0.0682]***	0.192 [0.0660]***
Observations	406	406	4477
R-squared	0.30	0.30	0.05
Control group mean	1.4516	0.132	0.1313
<i>Panel D. IV (DDJJ Average instrumented by random assignment)</i>			
DDJJ Average Q1-2008 to Q3-2010	-0.4577 [3.0767]	-0.0416 [0.2797]	-0.0416 [0.2720]
Constant	2.4020 [0.7020]***	0.2184 [0.0638]***	0.2184 [0.0621]***
Observations	405	405	4455
R-squared	0.12	0.12	0.02
Control group mean	1.4516	0.132	0.1313

Notes: Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 11.  
Number of credit inquiries (post treatment).

	Sum of Q1- 2011 to Q3- 2012	Average of Q1-2011 to Q3-2012	Multiple periods Q1- 2011 to Q3- 2012 (a)
<i>Panel A. ITT (OLS)</i>			
Random assignment	0.5252 [0.2223]**	0.0750 [0.0318]**	0.0685 [0.0304]**
Constant	2.0714 [1.2406]*	0.2959 [0.1772]*	0.3344 [0.1647]**
Observations	405	405	2849
R-squared	0.14	0.14	0.02
Control group mean	1.4946	0.2135	0.2124
<i>Panel B. Direct effect of employment on inquiries (OLS)</i>			
DDJJ Average Q1-2011 to Q3-2012	2.5884 [0.3468]***	0.3698 [0.0495]***	0.3540 [0.0435]***
Constant	2.3725 [1.2448]*	0.3389 [0.1778]*	0.3568 [0.1723]**
Observations	405	405	2835
R-squared	0.27	0.27	0.06
Control group mean	1.4946	0.2135	0.2124
<i>Panel C. Direct effect of earnings on inquiries (OLS)</i>			
Sum real earnings 01-2011 to 06-2012/1000	0.0444 [0.0062]***	0.0063 [0.0009]***	0.0062 [0.0008]***
Constant	2.6503 [1.2219]**	0.3786 [0.1746]**	0.3855 [0.1668]**
Observations	405	405	2849
R-squared	0.27	0.27	0.06
Control group mean	1.4946	0.2135	0.2124
<i>Panel D. IV (DDJJ Average instrumented by random assignment)</i>			
DDJJ Average Q1-2011 to Q3-2012	5.9785 [2.6372]**	0.8541 [0.3767]**	0.6609 [0.2858]**
Constant	2.3203 [1.2992]*	0.3315 [0.1856]*	0.3371 [0.1808]*
Observations	405	405	2835
R-squared	0.01	0.01	0.03
Control group mean	1.4946	0.2135	0.2124

Notes: Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Also we include pre treatment formal employment Q1-2008 to Q3-2010 and pre treatment number of inquiries Q1-2008 to Q3-2010. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 12.

Number of credit inquiries ITT (OLS-post treatment). Heterogeneous impact.

	Sum of Q1- 2011 to Q3- 2012	Average of Q1-2011 to Q3-2012	Multiple periods Q1- 2011 to Q3- 2012 (a)	Sum of Q1- 2011 to Q3- 2012	Average of Q1-2011 to Q3-2012	Multiple periods Q1- 2011 to Q3- 2012 (a)
	<i>Panel A. Female</i>			<i>Panel B. Male</i>		
Random assignment	0.1193 [0.2640]	0.0170 [0.0377]	-0.0002 [0.0357]	1.3182 [0.4769]***	0.1883 [0.0681]***	0.1916 [0.0555]***
Constant	2.8002 [1.3202]**	0.4000 [0.1886]**	0.4420 [0.1785]**	-2.4165 [1.9378]	-0.3452 [0.2768]	-0.0232 [0.2809]
Observations	279	279	1953	126	126	896
R-squared	0.16	0.16	0.03	0.40	0.40	0.08
Control group mean	1.6452	0.2350	0.2350	1.1935	0.1705	0.1678
	<i>Panel C. Age 18-24</i>			<i>Panel D. Age 25-30</i>		
Random assignment	0.8996 [0.3418]***	0.1285 [0.0488]***	0.1285 [0.0429]***	-0.0581 [0.2896]	-0.0083 [0.0414]	-0.0100 [0.0400]
Constant	2.2742 [1.0432]**	0.3249 [0.1490]**	0.3487 [0.1401]**	1.9483 [1.1334]*	0.2783 [0.1619]*	0.3745 [0.1374]***
Observations	229	229	1617	176	176	1232
R-squared	0.15	0.15	0.02	0.22	0.22	0.02
Control group mean	1.5146	0.2164	0.2143	1.4699	0.21	0.21

Notes: Covariates included are children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Panel A and B also include year of birth (dummies). Panel C and D include male. Also we include pre treatment formal employment Q1-2008 to Q3-2010 and pre treatment number of inquiries Q1-2008 to Q3-2010. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 13.

Effects on Asignación Universal por Hijo AUH ITT(OLS). Only female.

	Always Q1- 2011 to Q2- 2013	Multiple periods Q1- 2011 to Q2- 2013 (a)	Always Q1- 2011 to Q2- 2013	Multiple periods Q1- 2011 to Q2- 2013 (a)	Always Q1- 2011 to Q2- 2013	Multiple periods Q1- 2011 to Q2- 2013 (a)
	<i>Panel A. Female</i>		<i>Panel B. Female: Age 18-24</i>		<i>Panel C. Female: Age 25-30</i>	
Random assignment	-0.0233 [0.0266]	-0.0013 [0.0399]	-0.0564 [0.0221]**	-0.0310 [0.0438]	0.0192 [0.0587]	0.0305 [0.0696]
Constant	0.1750 [0.1457]	0.4020 [0.1914]**	0.0868 [0.0617]	0.0655 [0.1501]	-0.0287 [0.1401]	0.3503 [0.2183]
Observations	279	2790	147	1470	132	1320
R-squared	0.45	0.12	0.62	0.08	0.42	0.07
Control group mean	0.0968	0.2185	0.0656	0.1295	0.127	0.3048

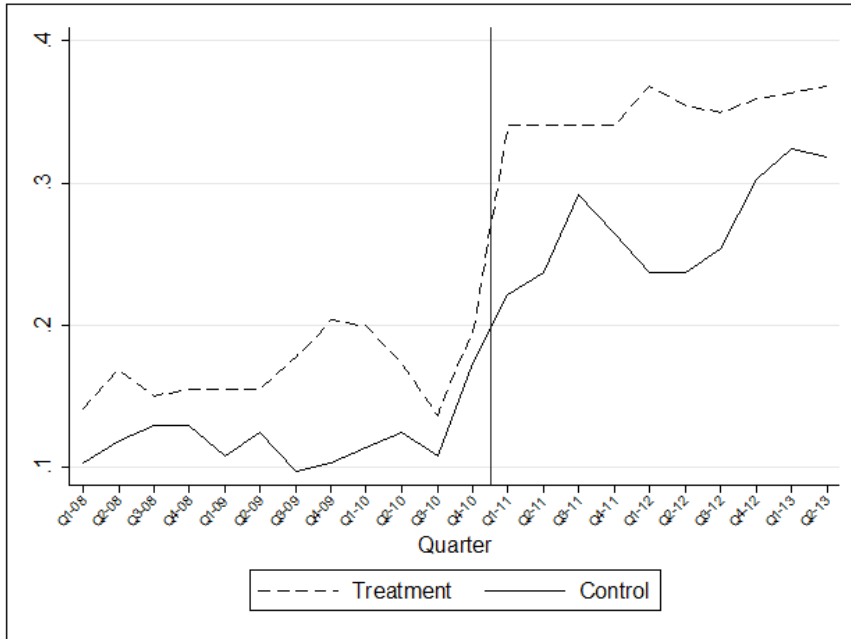
Notes: Covariates included are children in the household, year of birth (dummies), complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Post treatment Always employed also include pre treatment formal employment Q1-2008 to Q3-2010 and pre treatment AUH Q1-2010 to Q3-2010. Robust standard errors in brackets, (a) clustered standard errors (by individual) in brackets.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

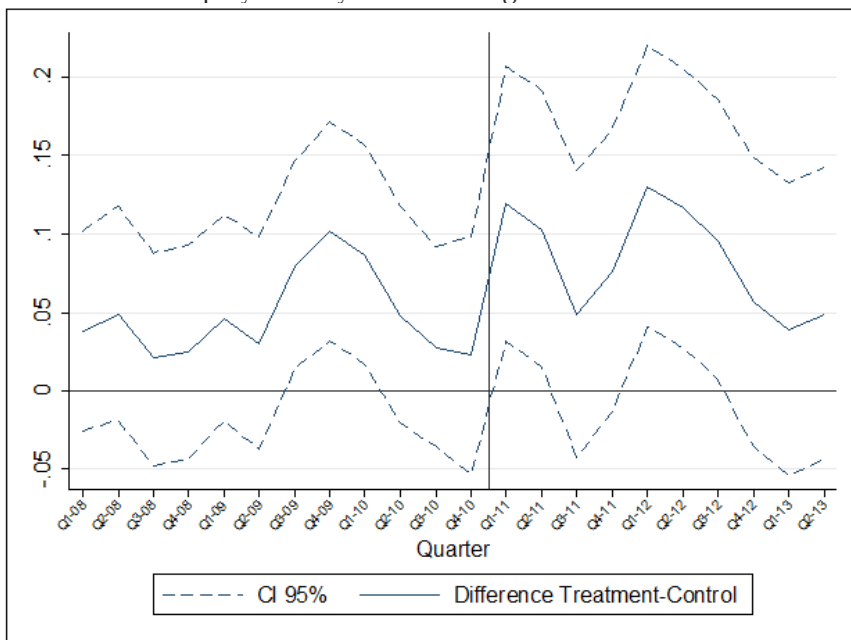
# Appendix C

## Graphs

Graph 1.  
Employment by random assignment.

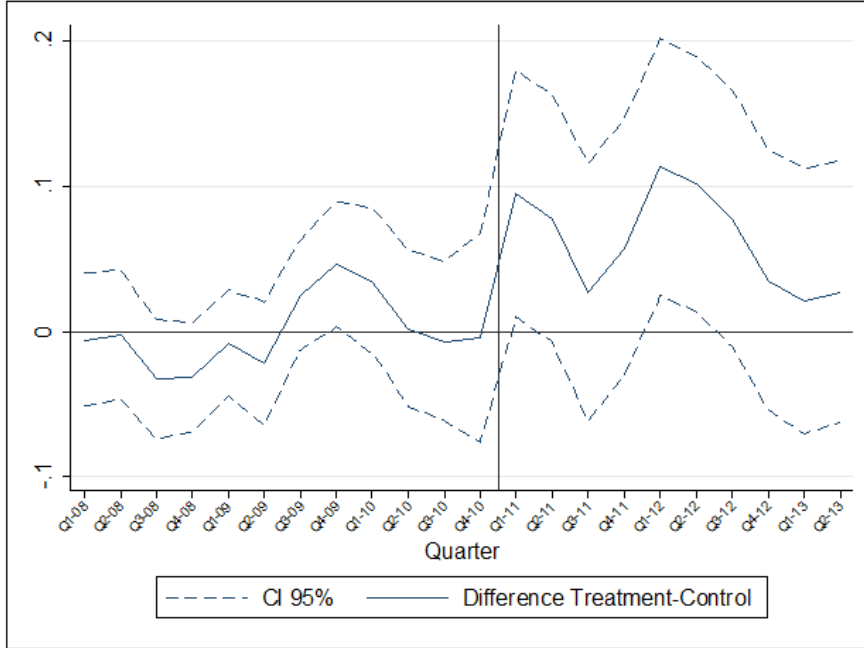


Graph 2.  
Differences in employment by random assignment.



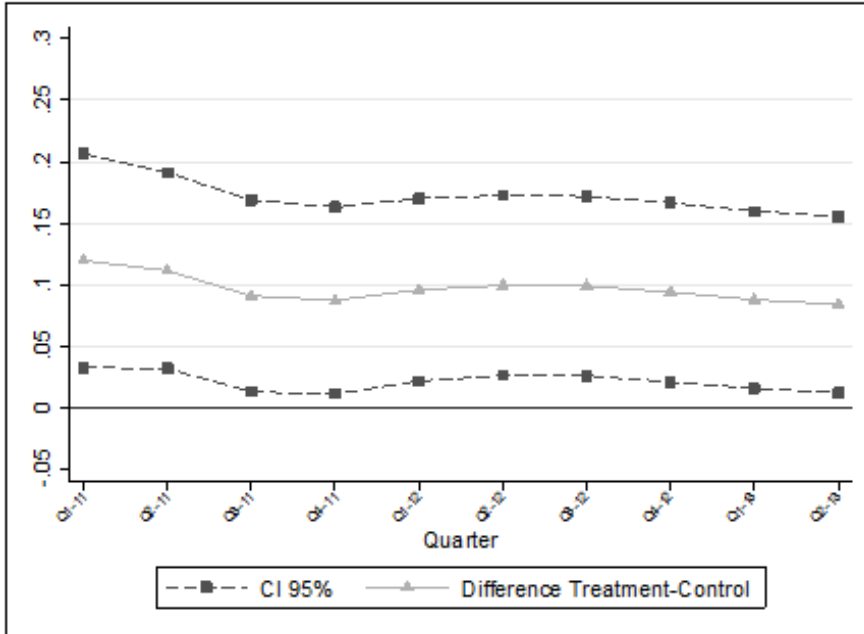
Graph 3.

Differences in employment (pre and post treatment) controlling with employment pre treatment.

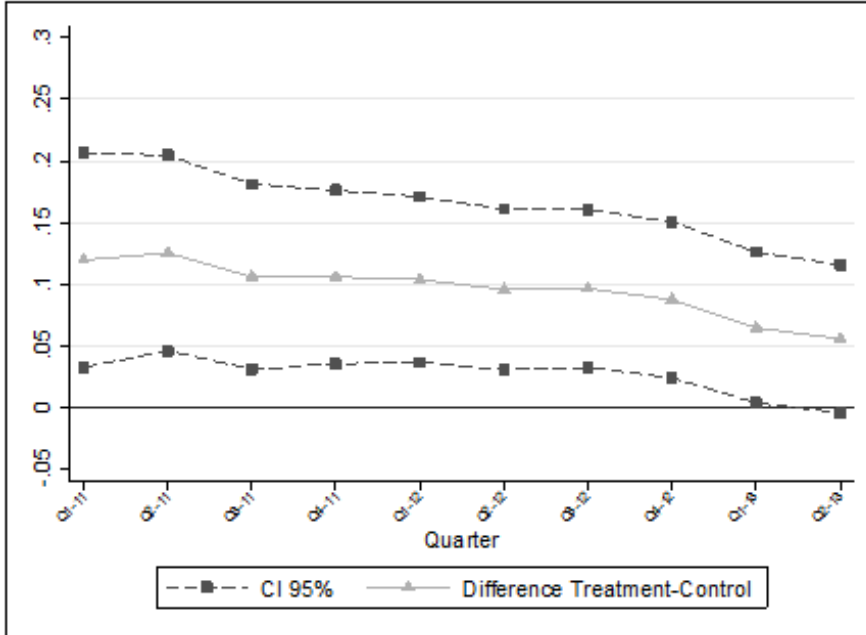


Graph 4.

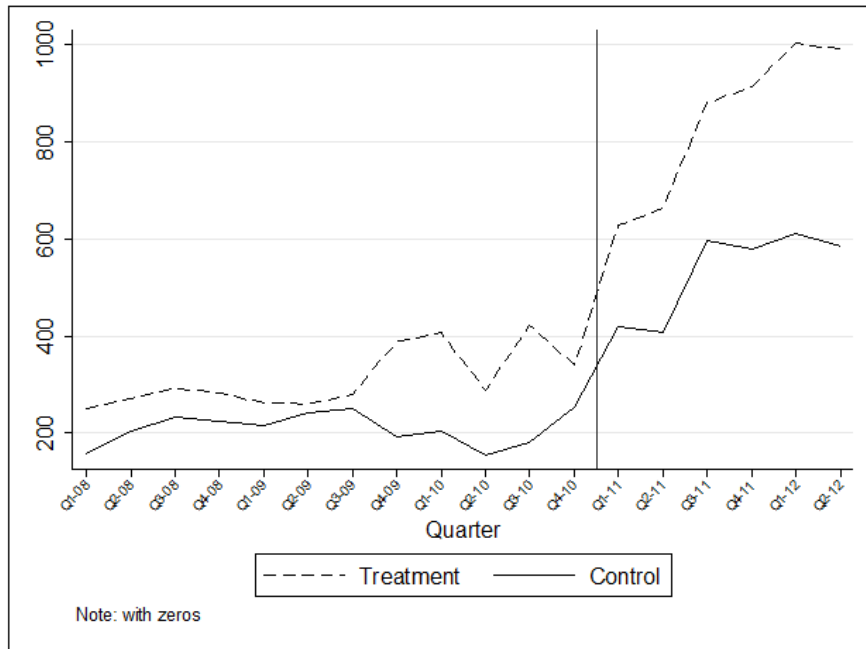
Differences in *average employed* post treatment in each quarter.



Graph 5.  
Differences in *always employed* post treatment in each quarter.



Graph 6.  
Real wages (with January 2011 as the base month) by random assignment.



Graph 7.

Differences in real wages (with January 2011 as the base month) by random assignment.

